LETTERS TO THE EDITOR.

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The Origin of the Cultivated Cineraria.

REFERRING to records of the history of cultivated Cineraria, I found (1) that considerable sports, or seedlings presenting notable and striking variations, arose in the early days of the "improvement" of the Cineraria; (2) that there is evidence that the improved varieties were of hybrid origin. I concluded, therefore, that Mr. Dyer's statement that our Cinerarias have been derived from *C. cruenta* "by the gradual accumulation of small variations" was misleading in two respects. As we have now had the benefit of a fuller statement of Mr. Dyer's case, I ask leave to explain why it is that I still hold to my original conclusion.

Meanwhile, however, Prof. Weldon, intervening, has offered an apparently sustained criticism of my evidence, which to those no better prepared may have a formidable look.

We will first examine some of Prof. Weldon's minor points. In preface let me say that I do not contend that *no* sports or named varieties have ever been believed to have arisen directly from *cruenta*, or from plants so-called (for, as Willdenow hinted,[1] the name may have been misapplied to hybrids in the past as now); and, indeed, I gave Drummond's words that his *cruenta* "sported freely from seed."

Something was made also of the wise caution which Burbidge gives in his general "Introduction" (p. 118), putting the reader on his guard against specific assertions as to the origin of hybrids. I mention, therefore, that I have received from Mr. Burbidge a letter warmly supporting the opinion given in the body of his book (p. 249) that the Cinerarias are of hybrid origin.

But now for what Prof. Weldon takes to be the real strength of his attack. He says that I omitted passages proving that according to contemporary opinion many of the named varieties cultivated between 1838 and 1842 "were not hybrids," but were "believed to be pure-bred *cruenta*." Upon what grounds this statement has been made, the reader shall now learn, not perhaps without astonishment.

The passage on which he chiefly relies is taken from Mrs. Loudon's article (*Ladies' Mag. of Gard.*, 1842, p. 111), to which I referred for the statement that in the writer's opinion the first important departure in the improvement of the Cineraria was made when Drummond hybridised *cruenta* with *lanata*. She goes on to say that, "since that time, numerous experiments have been made and hybrids raised" from several species. Next, that "some of the most beautiful Cinerarias now in our greenhouses, have been raised by Messrs. Henderson, Pineapple Place, particularly *C. Hendersoni* and the King, both raised from seeds of *C. cruenta*." This is the passage I omitted. Prof. Weldon says that this "passage clearly shows that in the writer's [Mrs. Loudon's] belief, the hybrids produced by Drummond and others, had not given rise to two at least of the named varieties of her time," and that she believed the King and *Hendersonii* to be descended from *cruenta* alone. This Prof. Weldon tells us is certain

Now, were we even bounded by the limit Prof. Weldon has set to his own researches on this question, we might hesitate to assume that whenever it is not expressly declared that a plant is a hybrid, we may be sure that the author thought it was pure-bred. As it happens, however, I can meet the charge with a weapon sturdier than the fine point of "dialectic." The answer is quite simple and curiously complete.

I shall now prove that both the King and *Hendersonii* were well known as hybrids both to Mrs. Loudon and to others. Let me point out:

(1) That the words say that the King and *C. Hendersonii* were raised from *seeds* of *cruenta*: as to the *male* parent, nothing is there said.

(2) That even if the evidence ended here, a discriminating reader might have suspected (what I shall presently prove) that Mrs. Loudon's *particular* statement about the King, *Hendersonii*, &c., is merely meant as an expansion of her previous *general* statement that since Drummond made his beginning numerous hybrids had been raised.

(3) That, were the matter doubtful, other passages in Mrs. Loudon's works prove this to be her meaning. For in *Ladies' Comp. to Flower-Gard.*, 1849 (s. v. Cineraria), she states, "the finest hybrids are *C. Waterhousiana*, *C. Hendersonii*, and the kind called the King." Again, in *Ladies' Flower-Gard., Greenho. Plts.*, 1848, p. 178, speaking of the woolly leaves, &c., of *lanata*, she says, "these peculiarities are found in all the numerous hybrids that have been raised from it. Perhaps the most ornamental of these is the hybrid called the King."[1] Of this, therefore, I presume Mrs. Loudon believed *lanata* to be the father, *cruenta* the mother.

(4) Lastly, that in order to have learnt that the King and *Hendersonii* were "between 1838 and 1842" considered to be hybrids, Prof. Weldon need not have gone far. He tells us he has read the articles on *C. Webberiana* (*Paxt. Mag.*, 1842, p 125) and on *C. Waterhousiana* (*ibid.*, 1838, p. 219), to which I gave references. Will it then be believed that in the first of these very articles *the King is referred to by name as a notable hybrid;* and that in the second article, "*C. cruenta*, var. *Hendersonia*" is with others named as *one of* "*the hybrids raised and grown by Messrs. Henderson, Pine-apple Place?*"

I do not know if it is wished that I should further refute Prof. Weldon's charge of "want of care in consulting and quoting the authorities." I am not unprepared to do so. I shall be glad to explain why Mrs. Loudon was probably right in substituting the name *tussilaginis* for *tussilagofolia;* to show why *Webberiana*, price 10s. 6d. (*Gard. Chron.*, 1842, p. 665), may be called a striking advance on its contemporaries, price 2s. 6d. (*Gard. Chron.*, 1842, p. 633), together with many other matters not yet treated of in this discussion.

My first objection to Mr. Dyer's statement was taken on the ground that there is historical evidence that sports, or seedlings presenting notable variations, occurred in the early days of the improvement of the Cineraria. To this, after reading his letters with great care, I do not find any specific answer. He tells us that the history as he gave it would be "in accord with general horticultural experience." It obeyed then a rule to the proof of which exceptions are indeed not lacking. He says, further, that to improve a plant the only safe way is by "selecting the minutest trace of change in the required direction," and "by patiently and continuously repeating the operation." Now this would be all very well if we knew nothing about the origin of the Cineraria; but against the evidence that seedlings presenting striking variations did as a fact arise, and against the historical evidence that Cinerarias, much as we know them, did as a fact come into existence within some twelve years, such *à priori* expectation is worth nothing at all.

To my second objection, that there is evidence that the chief start in the improvement of Cinerarias came as the result of hybridisation, Mr. Dyer has given more attention. He proposes to meet it by rejecting the whole of the historical evidence as unsound, and preferring the conjecture to which he says an inspection of the modern plants has led him. The historical evidence is to go because we are told certain horticulturists are ignorant men. I premise that this is not a principle which Darwin, whom Mr. Dyer would claim as his master, would have endorsed.

But before judging, let us try to consider what was the objective evidence on which the gardeners made up their minds that the new Cinerarias were hybrids. I may illustrate this by reference to a seedling now growing in the Cambridge Botanic Garden, to which Mr. Lynch, the curator, kindly called my attention. The case is of special interest in view of Mr Hemsley's objection that it requires skill and care to raise a hybrid in the Compositæ. It was with regret I learnt that this careful writer was not with me in this matter.

This seedling was raised from a seed of our plant of *lanata*, which was received from and is exactly similar to those at Kew.[2]

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[1] He says ("Enum. Pl. Berol.," 1809, p. 893) that Cinerarias are grown in gardens under the name *cruenta*, though really very different from it, having flowers almost like those of *lanata*. To these he gave the name *C. hybrida*. Moreover, from Bouché's experiment, we know that the seedlings of this *C. hybrida* were very variable.

[1] So famous a hybrid was the King, that I regret that I did not mention it in my first letter. I did not do so, as I found no coloured plate of it. Mr. John Fraser, of South Woodford, kindly informs me that he remembers it as the best of the woody sorts formerly grown. Its flowers were about the size of a penny, rays white tipped with purple, leaves downy and of a silvery hue on the underside. Its seedlings were unreliable.

[2] There labelled *Héritieri* (of DC.=*lanata*, L'Hér.). I note that though otherwise agreeing exactly with the *lanata* described by L'Hér., de Candolle and Webb, the inflorescence of these plants differs, being a loose corymb of some twenty heads, instead of the single flowered peduncle (*rami semper monocephali*, Webb) of the old authors. Whether this variation is known in wild plants, I cannot tell.

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In habit and size our seedling is not at all like *lanata*, but might be taken for a poor specimen of the common Cinerarias. In several characters it is intermediate between *lanata* and the latter. The stem is rather woody, less so than in *lanata*, but it is thick like those of garden kinds: petioles like *lanata* in having no auricles: leaves, nevertheless, large like those of garden kinds, the backs very woolly, but largely purplish, as in many cultivated sorts. Now this plant must be either (1) a sport from *lanata* in the direction of the garden forms, or (2) an accidental hybrid between *lanata* and one of the cultivated kinds growing in the same house with it (we have no others). The latter seems more likely—an opinion in which Mr. Lynch fully concurs.

Similarly Bouché (*Wittm. Monatss.* xxii. p. 298, orig. not seen, quoted from Focke, *Pfl. Mischlinge*, 1881, p. 201) says that a hybrid between *C. Webbii* (Schlz. Bipont.) and *cruenta* arose in the Berlin Botanic Garden as the spontaneous product of these species growing side by side.

It was, I think, on evidence like this that the parentage of the older hybrids was conjectured; but that Drummond and Henderson certainly—and possibly others—did make definite efforts to hybridise, cannot on the evidence be doubted. That these efforts went no further than the brushing of pollen of some species upon the flowers of others, I fully believe, and that on such evidence the *precise* parentage cannot be assigned is obvious. Nevertheless distinct seedlings resulted. In a few years, as the writer in *Paxt. Mag.*, 1842, p. 125, says (in an article urging to fresh efforts in crossing), this hybridisation "was the means of creating quite a novel and superior race." There were the new plants: how had they arisen? Those who doubt that these new kinds were hybrids must choose the other horn of the dilemma, and accept them as sports pure and simple.

That the historical records may contain errors, I am fully aware; but if they cannot be accepted in detail, should they be altogether rejected? We might perhaps reserve a doubt whether the King came precisely from pure *cruenta* fertilised by *lanata*; whether *cruenta* var. *lactea* was a hybrid between *cruenta* and *populifolia* (as de Candolle surmises); whether *Waterhousiana* was the offspring of true *cruenta* and true *tussilaginis*; whether Mrs. Loudon's statement that the species used were *cruenta*, *lanata*, *aurita*, *tussilaginis*, and *populifolia*, or Moore's belief that *cruenta* and *tussilaginis*, with perhaps *Héritieri* (= *lanata*), *maderensis* (= *aurita*), and *populifolia* ("Cross and Self-Fert.," p. 335, *note*), or Otto's similar declaration (Regel's *Gartenflora*, 1857, p. 66), or that of the *Jour. d'hort. Gand*, 1846, already given, should each be taken without hesitation as full and complete statements of the whole truth, but that they contain a substance of truth is hardly in question.

Against this Mr. Dyer offers nothing but an opinion derived from an inspection of certain modern plants. He who has confidence in the results of this method must suppose our knowledge of the laws of inheritance to be much more complete than I believe it to be. It is not the method Darwin used. Take a well-ascertained case. Who would know from inspection of the Himalaya rabbit that it came directly from the Silver-greys or Chinchillas? (See *Animals and Plts.*, i. p. 113.) It is unlike them, is of sudden origin, and yet breeds true.[1] To suppose that in cross-bred offspring given characteristics of the parents must be found, is to assume the precise question which in a discussion of organic stability is at issue. Let it be remembered that on the hypothesis of hybrid origin for our Cinerarias it is supposed that they result from several species and varieties, crossed not once only, but many times, in wholly irregular ways. Can it be seriously expected that any special resemblance to a given ancestor should be still traceable?[2]

My position then is this. We heard Mr. Dyer's statement; turning to the literature I found an entirely different account, borne out by copious and on the whole fairly attested evidence, pointing irresistibly to the conclusion that the Cinerarias are species which hybridise freely, and that our modern forms have arisen through such hybrid unions.

Mr. Dyer has well said that "if you take any statement that Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset." By the courtesy of an opponent I have been directed to a passage in "Cross and Self-Fertilization," 1876, p. 335, where (before describing experiments showing considerable self-sterility in the garden Cineraria) Darwin gives this definition of his material, "*Senecio cruentus* (*greenhouse varieties, commonly called Cinerarias, probably derived from several fruticose or herbaceous species much intercrossed*"). It seems, therefore, that in this matter also Mr. Darwin has, to use Mr. Dyer's words, "squeezed out" of the evidence "all that it would profitably yield."

Here I would fain leave the subject. But perhaps it may be suggested that though Darwin's Cinerarias were probably hybrids, our Cinerarias may not be their descendants. Such a suggestion involves the supposition that in some hidden place there was a thin red line of pure *cruenta* waiting for the moment when it should oust the hybrids. If this be seriously suggested, I shall ask where such a strain was kept, and what steps were taken to preserve its purity. In view of the evidence that chance blendings occur freely, to keep a pure strain would require some care. Until this has been proved, we shall not, I think, be wrong in supposing that each grower worked on the material his predecessors had created, and that our Cinerarias are the lineal descendants of the hybrids raised in the first half of this century,

In the course of this discussion, Mr. Dyer has treated me to some hard words, which I do not particularly resent. Whether I have deserved them is not, perhaps, for me to judge. But I will ask Mr. Dyer to point out when, on being asked for the facts upon which I have based a view, I have replied that that was a "matter for future collection." The facts I have been able to collect may be few, but by a study of the writings of my antagonists, I have not been able to add materially to their number.[1] W. BATESON.

St. John's College, Cambridge, May 26.

IT has been pointed out to me that my remarks on Mr. Bateson's account of the Cineraria have been interpreted in a sense of which I did not dream when I wrote them.

I wish, therefore, to say that, although I do not believe Mr. Bateson's reading of the passages I quoted to be the true one, yet I have never questioned his sincerity in suggesting it, and I am pained to find that I have seemed to do so.

May 24. W. F. R. WELDON.

Boltzmann's Minimum Function.

I GATHER from Mr. Culverwell's last letter (NATURE, April 18), and Mr. Bryan's (May 9), that we may regard the following conclusion as established, namely, the proof of the H theorem, for any system depends on a certain condition (A) being fulfilled among the coordinates and momenta of the molecules forming the system. Considering these as elastic spheres, and using Dr. Watson's notation, $f dp_1 \ldots dq_3$ is the chance that a sphere shall have for coordinates and momenta $p_1 \ldots p_1 + ap_1$, &c., and $F dP_1 \ldots dQ_3$ the chance that another sphere shall have $P_1 \ldots P_1 + dP_1$, &c. The condition required is that f and F are independent, even for two spheres on the point of collision.

Otherwise we may express it. Let there be n spheres in space S. Let us suppose Mr. Culverwell to assign to each its position at time $t = 0$, and Mr. Bryan to assign independently to each its component velocities. Then the condition A is fulfilled when $t = 0$.

We can then prove that when $t = 0 \ \frac{dH}{dt}$ is negative, or, as Herr Boltzmann would have us say, is more likely to be negative than positive.

Now arises a question which seems to me to deserve consideration. Assuming our system to be finite, and to be left to itself unaffected by external disturbances, does it necessarily

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[1] To Mr. James, of Farnham Royal, a celebrated grower, and to his foreman, I am indebted for several interesting points, and especially for the following: Formerly blue self-coloured Cinerarias were scarce in his strain, but some years ago, he introduced some plants of a French strain. After this, and presumably as a result of the cross between his own and the French kinds, there appeared a strain of blue selfs. These, though shy seeders themselves, transmit their peculiarity so strongly that they have to be kept in a house apart, for fear that their character should assert itself to the exclusion of everything else.

[2] In order to meet Mr. Dyer on his own ground, I have assumed, what I cannot admit, that in none of the various modern strains traces of the different parent-species appear.

[1] It has been impossible for me to incorporate in this letter all the mass of information which has been most generously sent me by correspondents since this controversy began. It is suggested that I should point out that Mr. Dyer's use of the word "feral" to mean "wild" is not usual. A correspondent tells me that it was probably first used in the special sense of "run wild" by Hamilton Smith, *Nat. Libr.*, *Mammalia*, 1839, ix. p. 92. It has since been so used by many authors, especially Darwin, *An. and Plts.*, i. p. 117, &c.

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